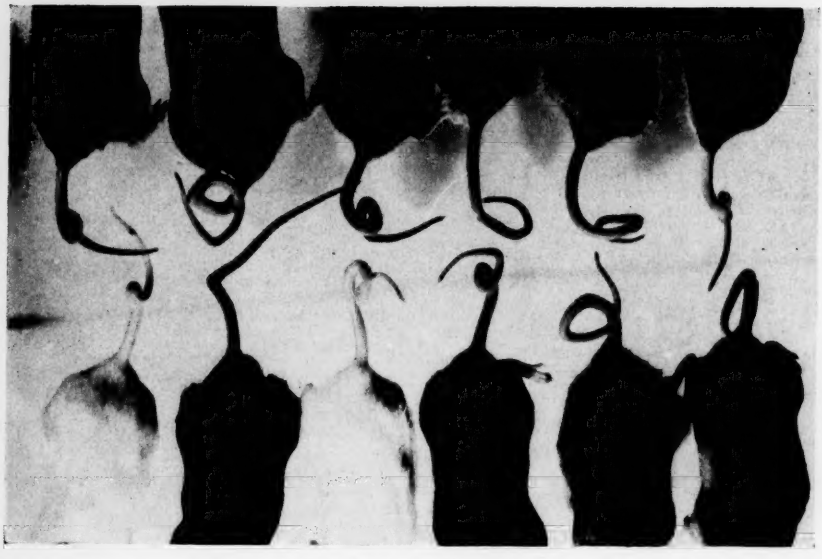
The Journal of HEREDITY



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Sex-Mixed Honey Bees

Human Isolate Essay Contest

The Disintegration of Civilization

Finalism—Inbred Progeny of Vitalism

The Soybean's Skin and the Three Bad De-Genes

VOLUME 40 DECEMBER, 1949 NUMBER 12

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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The

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HUMAN ISOLATE ESSAY CONTEST

$2,000.00 in Prizes Offered for Studies of Size of Human Mating Groups

THE Council of the American Genetic Association announces an essay contest which will explore some aspect of the problem of the human mating isolate in the United States in its genetic and eugenic implications.

The prizes offered are as follows:

First prize: $1,000.
Second prize: $500.
Third to seventh prize: $100 each.

The Topic: The Problem of the Non-Geographic Isolate

I. *Definition*: What is a non-geographic isolate? What are its characteristics? How do we prove its existence?

II. *Delimitation of an Isolate*: How are the numerical and genetic limits of an isolate best established? Some possible methods suggested are:

1. By physical traits;
2. By pedigrees;
3. These or other methods to be described and delimited by application of statistics;
4. Other methods to be developed by competitors.

III. *Causation*: Is the selection (or other force) which establishes and maintains an isolate principally to be attributed to one (or all) of the following:

(*Continued on page* 311)

BEE WITH A COSMOPOLITAN OUTLOOK

Figure 1

This bee sees the world from both the male and the female standpoints. The right compound eye, the right antenna, and the right mandible are male. The two right ocelli are in the male position. The left compound eye, the left antenna, and the left mandible are female, and the left ocellus is on top of the head in the female position.

GYNANDROMORPHIC HONEY BEES*

WALTER C. ROTHENBUHLER, MARTIN S. POLHEMUS, JOHN W. GOWEN, AND O. W. PARK
Iowa State College, Ames, Iowa

FOCUS OF HYMENOPTERAL FRUSTRATION

Figure 2

This individual's head is constructed to see both sides of the sex-question (see Figure 1). The body is all of the female sex. The male parts are assumedly haploid, the female parts diploid. In some cases, the male mosaic areas show characteristics of fathers that genetic theory denies them. This situation offers alluring leads for future research.

HONEY bees having combinations of male and female body tissue (gynandromorphs) have been observed and reported over the last 150 years by beekeepers of various countries. Occasionally the reports of large numbers of such aberrant sex types have crept into the scientific press and stimulated biological hypotheses for explaining the sex machanism.† Genetic experimentation on the problem has been prevented or handicapped because of inability to control matings in honey bees. This hinderance no longer exists, since the Watson technique of artificial insemination, as improved by Nolan, Laidlaw, and Mackensen,[2] makes controlled matings possible.

In September, 1948, at the Iowa Agricultural Experiment Station Apiary, a

*Journal Paper No. J 1679 of the Iowa Agricultural Experiment Station, Ames, Iowa. Project No. 1166.

†See Literature Citations 1, 3, 4, 5, 6.

BILATERALLY DIVIDED THORAX

Figure 3

The head is completely male; the thorax is male on right side, female on left side. The abdomen is completely female.

queen bee of an ivory eye stock (this mutant obtained from Dr. Lloyd R. Watson in 1947) was found to be producing large numbers of gynandromorphic bees. The production of such bees has continued to the present (August 1949).

Further search has revealed the presence of these individuals in colonies headed by two other queens, both related to the original gynandromorph producer.

The sex mosaics found in these colonies have shown various combinations of male and female tissue. Drone heads on female bodies have been conspicuous. Heads approximately half male and half female with the line of division either horizontal or vertical have been frequent, as have bilaterally divided thoraxes and abdomens. Vestigial stings have been noted on otherwise drone-appearing abdomens.

In addition, gynandromorphs mosaic for eye color have appeared, with one colony providing particularly interesting specimens. The queen was homozygous for recessive ivory eye, and was artificially inseminated with sperm from one ivory-eyed drone and one black-eyed drone. Several gynandromorphs arising from this mating had black drone eyes. Inasmuch as it has been generally believed that the mating of the queen has no effect on the genetic makeup of her drone progeny, these individuals are of particular significance, one possibility being that the black drone eyes were the result of fertilization. Controlled matings and embryological studies to analyze the genetics and the developmental physiology of gynandromorphic honey bees are in progress.

Photographs of certain of these sex types accompany this paper.

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✦

Human Isolate Essay Contest

(*Continued from page* 307)

1. Physique?
2. Race?
3. Religion?
4. Family?
5. Social position?
6. Occupation?
7. Wealth?
8. Other Criteria?

IV. *Illustration*: Contestants will illustrate and justify their answers to the above questions, and/or to any other factors which they attempt to establish as significant, by a factual study of an isolate not primarily geographic in the United States. Selection of material to explore this problem is left to the contestants.

The Mating Isolate Concept

In this contest the term "mating isolate" is used in a restricted sense, as is "non-geographic." Thus if it could be shown that the traditional "railroad tracks" delimited a social as well as a geographic boundary, the social delimitation would be acceptable as a definition.

Such generalized concepts of isolate boundaries, as an over-all delimitation of mating isolates of different racial groups would lie outside the scope of this contest. While there is no desire to be arbitrary in delimiting exactly the area in which an isolate in the sense intended might exist, it would be expected to lie within a city and its environs, a school or voting district, a county, or at most a state. The boundaries of an isolate might, of course, include parts of two, or more, such political (or even geographical) areas. In judging the es-

(*Continued on page* 312)

says, the precise of delimitation of a small, well-defined isolate would be expected to achieve a higher rating than a vague and general delimitation of a larger unit.

The concept and definition of the mating isolate as a factor in determining the liklihood of the fixation of favorable or unfavorable mutations has developed from the pioneer work of Haagedoorn, Wright, Fisher, Haldane, and others. The concept that the frequency of cousin marriages is a measure of the size of the mating isolate has been developed principally by Dr. Gunnar Dahlberg of the Swedish Institute of Race Biology. In his recent analysis of this problem (*Mathematical Methods for Population Genetics,* Interscience Publications, New York, 1948), Dr. Dahlberg writes as follows:

> For our analysis we use as starting-point that, as regards heredity, a person has six different possibilities. *Firstly,* he can marry (and propagate) at random. If marriage is contracted in this way, panmixia is said to obtain. *Secondly,* he can refrain from marrying or propagating to a greater or lesser extent than the average. If a group of character-bearers behaves in this way, we have selection. *Thirdly,* a character-bearer can contract a blood marriage. If such marriages occur to a greater extent than a chance estimate would indicate, inmarriage is said to obtain. *Fourthly,* a person may be more inclined to marry character-bearers of one certain kind than of other kinds. The most important possibility here is that character-bearers of a certain kind have a greater tendency to marry another than chance dictates. This is called assoratative mating. *Fifthly,* a person who gets married has not the possibility of marrying over a whole population. . . . This means that a population contains isolate boundaries. . . .
>
> . . . In certain cases the boundaries [of the isolate] are sharp, in others vague, and it may happen that there are no real limits at all. In spite of this, a given person has greater prospects of marrying persons who live at a slight or a moderate distance, and, practically speaking, no prospects of marrying at a great distance. . . .
>
> To clarify the relationship between isolate and blood marriages we will imagine a definite isolate, e.g. an island, on which the population rapidly grows. This means that if panmixia obtains the marriages between cousins will decrease in frequency. Notwithstanding this, the frequency of character-bearers will not be changed. This is due to the fact that more distant degrees of blood marriages increase in frequency, and this balances the decreased frequency of the cousin marriages. If we could register blood marriages of a more distant degree (than first cousin marriages), it should be possible to regard the process from the point of view of the isolate. This aspect holds for a number of problems of very great import.
>
> Actually, very important differences between the isolates are to be expected for rare characters. The isolates naturally vary very greatly in size in a population. The one extreme is the large towns, the other small villages in a region of sparse population. . . . The probability for a mutation to reach a moderate frequency by chance is naturally far greater in a small isolate than in a large one. For this reason rare defects are found primarily in small isolates, in remotely situated villages, where as a rule comparatively primitive conditions prevail and where marriages between cousins and other relations have a high frequency.

In the United States, most of the work on mating isolates has been based primarily on geographical isolation. That of Alexander Graham Bell, on the "Formation of a Deaf Variety of the Human Race" on Martha's Vineyard is a classic. Even there, however, important non-geographic factors were at work. The work of the late Dr. William Allen of Charlotte, N. C. (JOURNAL OF HEREDITY, September, 1942, p. 331), showed that in the North Carolina communities studied by him, church membership delineated rather sharp isolate boundaries. In one instance 76 per cent of all marriages were between members of the same congregation.

Judges

A panel of three judges will consider the essays submitted and choose the winners. Their decisions will be final. One judge shall be a specialist in each of the following fields: genetics; statistics; population. The judges will be announced shortly.

Participation

Contestants intending to participate must make formal application to the American Genetic Association, 1507 M Street, NW, Washington 5, D. C. In their application they should signify

(*Continued on page 324*)

VITALIST EVOLUTION

A Review

TH. DOBZHANSKY

GREAT advances in our understanding of evolution have been accomplished within the past two decades. The work of Darlington, Dubinin, Fisher, Huxley, Mather, Mayr, Muller, Patterson, Rensch, Schmalhausen, Simpson, Stebbins, Timofeeff-Ressovsky, Sewall Wright, and many others has led to a synthesis of contributions to evolution theory made by various biological disciplines and to emergence of a comprehensive view of the evolutionary process. Unquestionably, many fundamental problems remain to be solved, and the recent developments ought to be regarded as merely a prologue to the advances to come. Yet there can be no reasonable doubt that our day has seen the greatest progress in evolutionary biology since Darwin's *Origin of Species*.

The modern theory of evolution has not, however, proved as convincing to all biologists as some of its creators fondly hoped it might be. The vituperative hostility of Lysenko and his partisans may be dismissed as based on ignorance and bad faith. But one cannot disregard the opposition coming from so distinguished an investigator and scholar as Goldschmidt. Another group of opponents are the so-called finalists, who are the modern successors of the now defunct vitalists. The eminent French biologists, Cuénot[1] and Vandel[4], have recently published treatises on evolution written from the finalist point of view. The popularly written finalist book of Lecomte du Noüy[3] has achieved a best seller success despite its many gross errors in matters of fact as well as glaring faults of logic. These dissenting voices deserve attention and refutation, otherwise they are bound to mislead many sincere seekers of truth among non-biologists, to whose opinions biologists cannot justifiably be indifferent.

Mechanism is a theory which assumes that life can be resolved into physical and chemical components, similar in kind to those which occur also in inanimate nature. Vitalism negates this assumption and contends that life is the manifestation of a special principle, to which the name "vital force" has been given. Vitalists were a majority, or, at any rate, wielded much influence, among biologists up to the end of the past century. However, with the progress of biology and especially with the development of experimentation, more and more living processes received physico-chemical explanations. The sphere of action of the supposed vital force was shrinking rapidly. Eventually, mechanism became accepted by most biologists as a superior working hypothesis and, by some, even as a self-evident truth. Early in the current century, Driesch's[2] brilliant writing made vitalism again attractive to a few biologists; but it soon became evident that Driesch's entelechy was but a fancy name for the old vital force.

The fatal weakness of the old vitalism lay in the fact that the supposed vital force was assumed to direct the flow of physico-chemical processes which admittedly took place in all living bodies. Now, any force so deeply embedded in the physico-chemical matrix must of necessity be another species of physico-chemical agency, let us say like electricity, although confined to living matter. For this reason, spiritualists and metapsychologists, who accept the most startling "phenomena" like alleged formation of bodies from "ectoplasma" generated by a "medium," are often quite consistent, although unorthodox, materialists. Similarly, all kinds of "supernatural" phenomena which are alleged to influence material processes must *ipso facto* involve physico-chemical forces, although of unusual kinds. Some supernaturalists are oddly materialistic.

Vandel admits that "vitalists are now no more than curious relics, and one can-

not at present regard the living and the inanimate as two entirely different states of matter unconnected by bonds of descent." But vitalism has not expired without leaving progeny. This progeny, called finalism, has however withdrawn from the level of pedestrian biological research to the lofty heights of metaphysics. According to Vandel, "life has two faces, one of which belongs to inanimate matter and consequently to mechanism, the other is directed toward spontaneity, invention, and creation. It is in this double prespective that one obtains an adequate image of life, which permits correct interpretation of its diverse and multiform aspects." The old vitalism was confounded when one life phenomenon after the other was explained without invoking any forces other than those known in inanimate nature. But no physiological discovery can demonstrate the truth of falsity of the assertion that organic evolution, and evolution of the Universe as a whole, happened for the purpose of producing the human species, and that "all else are by-products, slags, left over after its production. This is why the human law is also the law of the world."

Metaphysical speculation may or may not be within the competence of a biologist to evaluate on philosophical grounds. But inasmuch as philosophical and metaphysical doctrines tend to influence both the kind of research undertaken and the interpretation given to the results, a biologist is entitled to judge the conclusions reached by his philosophical colleagues regarding biological problems.

Does Genetics Help to Explain Evolution?

Finalism postulates that life is a primary attribute of all matter. According to Vandel, "Matter was at its origin rich in power and in unrealized possibilities, able to engender the organic as well as the inorganic, the living and the inert. In giving birth to life, matter passed to the latter the essence of its creative energy, and this effort reduced matter to a degraded relic, devitalized, and having lost most of its ancestral qualities. Such is the matter which is being studied by physicists and chemists." Within the living world, "the development of the nerve system and of psychism represents the fundamental tendency of evolution in animals. Psychic development constitutes the surest criterion of progressive evolution." Botanists will note that this reduces the plant kingdom to "an immense regressive line."

The scientific reputation of Professor Vandel is based chiefly on his research in cytogenetics. It may seem strange, therefore, that in his book he is occupied with paleontological aspects, almost to the exclusion of the genetic aspects of evolution theory. This is, however, explained in the following astounding statement: "Mendelian heredity discloses to us one of the most mechanical aspects of life, that in which living matter lost all initiative and took refuge in implacable routine." This opinion seems quite unconvincing to the reviewer and Vandel did not feel it necessary to indicate the reasons which compelled him to adopt so startling a view. Does he simply mean that since there is no need to invoke finalism to explain Mendelian heredity it must be degraded to a level of implacable routine?

The finalism of evolution is revealed by the "cycles of evolution" which, according to Vandel, are demonstrated by paleontological evidence. A cycle consists of a "period of preparation," when organisms are rare, undifferentiated, and of small body size; and explosive "evolutionary crisis" when higher forms of organization are arrived at; a "diversifying evolution," which makes the new group of organisms widespread and subdivided into more or less numerous variant types; and a "phase of senescence and death" which leads to extinction of the strain. Genetics, we are told, has shown itself incapable of explaining the facts brought to light by paleontology. This opinion, at least when categorically expressed, is demonstrably false. Professor Vandel must be unfamiliar with the work of genetically-minded paleontologists, especially G. G.

Simpson, who have made it at least probable that the known principles of genetics are sufficient to account for all well established findings of paleontology.

Adaptation

Most modern theories of evolution consider adaptation to be the prime cause of evolutionary change. Organisms become altered and evolve chiefly in the process of becoming better able to survive and reproduce in the environment in which they live. Finalists rate the adaptive aspects of evolution less highly. Adaptation, according to Vandel, amounts to solution of problems "which seem to be borrowed from the mechanized habits of inert matter." The process of speciation is mostly regressive evolution, and the preoccupation of evolutionists with studies of speciation is blamed for their failure to discern the finalism which underlies evolutionary progress. There exists "a difference, one might even say opposition, between adaptive evolution and progressive evolution."

Nevertheless, Vandel devotes considerable space in his book to consideration of the problem of adaptation, while the book of Cuénot is devoted almost entirely to this problem. The theory of the origin of adaptation through selection of mutations and gene combinations suitable in different environments is dismissed by Vandel in the following characteristic paragraph:

> For the Darwinists, adaptation occurs outside the organism; it is a product of natural selection and competition for life; the organism plays in the genesis of adaptations only a purely passive rôle. It is understandable that this interpretation could satisfy Darwin at a time when factors of development were quite unknown. But it is less understandable to find that, despite the magnificent findings of experimental embryology which have thrown quite new light on development and on correlations, numerous biologists, particularly of Anglo-Saxon nationality, still remain firmly attached to the principles of Darwinian interpretation.

It is regrettable that neither Cuénot nor Vandel (not to speak of du Noüy) gave in their books a statement of the modern version of Darwinian interpretation of the genesis of adaptation by natural selection. Such a statement would have been in order, since it would help the authors to state and to explain the nature of their objections to this interpretation. This reviewer can find in neither book a mention of the experiments which show that mutants and mutant combinations which are unfavorable in some environments may be neutral or favorable in others; of the prodigious power of the sexual process to bring forth new gene combinations to be tried out by natural selection; of observations on the action of selection in nature; of experiments on artificial populations and on modification of gene frequencies in them; of the mathematical models of evolutionary processes evolved by Sewall Wright; and several other very pertinent topics.

Cuénot spends about half of his book presenting numerous and interesting examples of apparently adaptive structures which are supposedly too complex to have arisen "by chance," and of other complex structures that appear to be useless to their possessors, and thus could not have arisen by natural selection. This leads to the conclusion that these structures must have arisen by a process other than selection. Such a conclusion might have carried some weight when population genetics was in its infancy; it is much less serious now since we know that small selective advantages or disadvantages which are difficult to detect by observation may nevertheless be effective in moulding the genotypes of populations.

Cuénot and Vandel fail to offer an explanation of the origin of adaptations that would be more satisfactory than is the modern theory of natural selection. They believe that "the genesis of adaptations remains almost completely unknown" and that "the genesis of adaptations, just as the phylogeny, is not an object of experimentation." But, if so, how does Vandel know that "adaptation is the same thing as organization," "adaptation is an active reaction of the organism to the external milieu," "adaptation represents an extension of the power of organization characteristic of

living matter," etc? Cuénot attributes "to the germ cell a kind of combining intelligence, an immanent power equivalent to intentionality." Now, the reviewer submits, this is equivalent to assuming the existence of the old vital force, which both Cuénot and Vandel themselves consider not a satisfactory way out of the difficulty.

Has Evolution come to an End?

If evolution was induced by a Purpose, and if this Purpose were to produce Man, then evolution has fulfilled its task and is no longer needed. According to Vandel, "the evolution of the animal kingdom seems to us now terminated. To be sure, the microevolutions, the speciations, the regressive evolutions will go on for a long time; but these manifestations of evolutionary senility will not change the final result. Progressive evolution of the animal world, which has created the great organic types, has arrived at a stop." Neither is there much hope for evolutionary progress of the human species: "The level of human intelligence and of the psychic faculties is set by the human nervous organization. Men of genius, saints, and heroes represent probably the highest summit which humanity can ever reach." The last sentence expresses the most profoundly pessimistic conclusion ever made, to the reviewer's knowledge, supposedly from biological data. Fortunately, Darwinistic interpretation of evolution makes this pessimism unnecessary. There is no compelling reason to think that human evolution may not eventually lead to a superhuman level.

Biological Evolution and Cosmic Evolution

It is only fair to say that the vitalist-finalist current of thought has not thus far made any contribution of great value to the theory of evolution. With all due apologies to the finalist colleagues, the reviewer must confess that he cannot get away from the suspicion that the finalists who have written books on evolution were not too familiar with the genetic theory which they so precipitately and uncompromisingly rejected. There is nothing to be gained, either scientifically or philosophically, by attributing a "psychoid" to the germ cell (Cuénot), or by supposing that progressive evolution was impelled by a mysterious force which strived to produce man (Vandel).

And yet it would be narrow-minded to dismiss the whole finalist movement as a mere aberration of scientific thinking. The reviewer feels that there is much wisdom in these words of Vandel:

> Let us recognize very frankly that the problem of evolution does not fit into a strictly scientific frame. To envisage evolution solely in its objective aspects means to mutilate it, to renounce understanding of its essence, and to misjudge its true significance. Evolution has metaphysical projections which we cannot ignore.

Indeed, biological evolution is an integral part of the evolution of the cosmos. Human curiosity will not be satisfied until it can envisage the entire evolutionary perspective. Several evolutionary crises of transcendental significance have taken place in the history of the cosmos: genesis of matter; origin of a self-reproducing, i.e., living unit, a gene, from non-living substances; appearance of man with his powerful intellect which permitted genesis of culture and control of environment by invention instead of by biological adaptation. A Christian would add to these the life of Christ. We are as yet unable to discern the fundamental unity of the above series of events. But Vandel is probably right when he says that "emergence of a new evolutionary level is the result of a pressure which the representatives of the inferior level exert to break their own limits," and consequently that "Man does not really exist except when he struggles against his own bounds."

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INHERITANCE OF DEFECTIVE SEED COAT IN SOYBEANS*

HOU-LEE LIU†

IN commerce, soybeans with defective seed coats command a lower market value than normal beans. On the agronomic side, defective seed coats are objectionable because of the readier entrance of water and infection by fungi. The vitality of such seeds is also lowered. Hence, a study of this character has both theoretical and practical aspects.

Literature Review

As first reported by Stewart and Wentz,[2] beans with defective seed coats were produced from a cross between two types with non-defective seed coats, a ratio of 15 normal to 1 defective being obtained. They found no defective seeds carrying *I*, a gene which inhibits black and brown pigment on the hilum and seed coat. Hence they considered that it prevented defectiveness in the seed coats. However, Nagai[1] found an F_2 ratio of 9 defective to 7 normal from a cross between plants with normal and defective seed coats. In crosses between cultivated and wild types, Ting[3] found only a single gene difference for defective seed coats, normal being dominant. Woodworth[4] suggested the symbol *de* to symbolize defective seed coats.

Stewart and Wentz[2] also found that *t* (gray pubescence) is completely linked with *de*. This was later confirmed by Woodworth[4] and by Woodworth and Williams.[5]

In a study of another kind of defective seed coat, designated de_2, Woodworth and Williams[5] found linkage between it and P_2 (glabrousness) with two per cent of crossing over. Defectiveness caused by de_2 is perhaps the same as that investigated by Nagai.[1]

Materials and Methods

All materials were supplied by Professor C. M. Woodworth. The crosses were made by the writer during 1945 and 1946. Some F_1 plants were grown in the greenhouse, but most of them and all F_2 and F_3 generations were grown on the South Farm of the University of Illinois from 1945 to 1947.

Strains used as parents are designated "T" (type) number or "P.I." (Plant Introduction) number except in the case of named varieties.

General Description

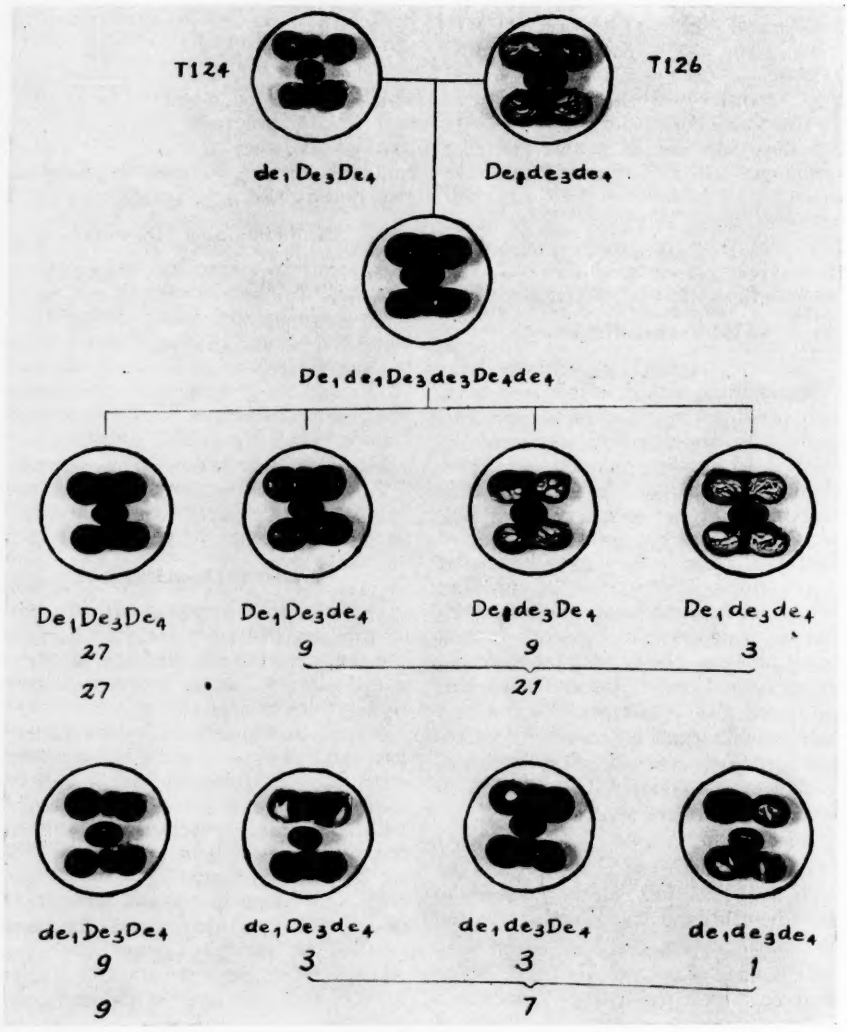
In order to facilitate a further study of this problem, the writer prefers to use the following classification, as there is a distinct difference between the two types of defectiveness:

Type I is represented by the variety *nao* and by a "multiple recessive strain."‡ Seeds usually have a buff or imperfect black seed coat accompanied with *t* for gray hair color. The pattern of the defective seed coat is quite irregular, as shown in Figure 5*E-F*. Some seed coats split along the dorsal surface exposing the cotyledons; and they are cracked in various degrees over the whole surface. Since the cracking is confined to the outer layer of the seed coat,

*Contribution from the Division of Plant Breeding, Department of Agronomy, Illinois Agricultural Experiment Station, University of Illinois, Urbana, Illinois, published with approval of the Director of the Station. Part of a thesis submitted in partial fulfilment of the requirements for the Ph.D. degree in Agronomy, Graduate College, University of Illinois.

†Graduate student, Division of Plant Breeding, Department of Agronomy, University of Illinois. The writer wishes to express his sincere appreciation to Professor C. M. Woodworth of the University of Illinois, under whose direction and encouragement the present study was initiated and accomplished, and by whose suggestions, corrections have been made in preparing this manuscript. Special thanks are also due to Dr. L. F. Williams, Federal Soybean Laboratory, U. S. Department of Agriculture, for his valuable suggestions and informative discussions.

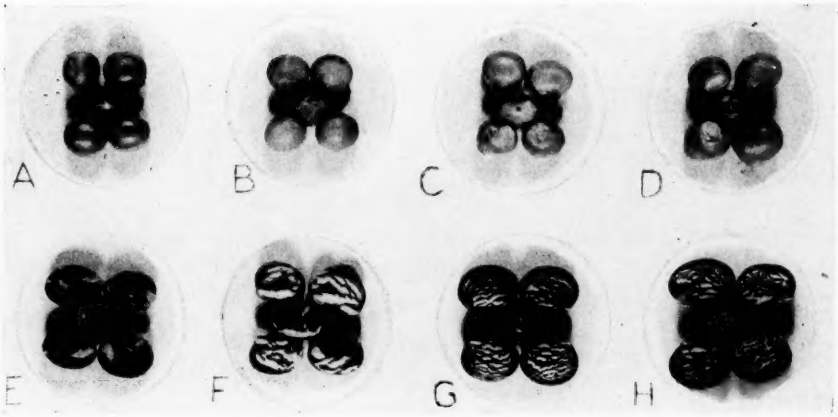
‡So named because recessive for a number of genes, as *r, t, de, l, dt.*



A SOYBEAN CROSS

Figure 4

The purest strains T124 and T126. The normal seeded F_1 and the segregant forms in F_2 are shown. Because several genotypes are indistinguishable the classical F_2 ratio becomes 27 : 21 : 9 : 7.



NORMAL AND DEFECTIVE-SKINNED SOYBEANS

Figure 5

The first row shows commercial types with entire seed coats, *A*—Lincoln variety and *B*, Richland. The varieties Chief (*C*) and Dunfield (*D*) have seeds showing more or less cracking of the seed coat. The defective types shown on the lower row are the nao variety (*E*), and the multiple recessive type of strain 3-797 (*F*). These are classed as Type I. Type II seed are shown at *G* (P.I. 82229), and at *H* (T.126).

the inner layer can be seen through the cracks. Usually, seeds with imperfect black (mottled black and brown) coats are cracked less than seeds with buff coats.

Type II is represented by two strains: T126 and P.I. 82229. No matter what kind of seed coat and hair color they may possess, seeds of this type always show the net-like cracking on their seed coats, as shown in Figure 5*G-H*. Such cracking is quite regular in distribution on the surface of the seed coat. It appears on both sides of the seeds and is never found on the dorsal edge. Since the white inner layer of the seed coat is exposed by the cracking, these types have been referred to as "black and white," "brown and white," or "yellow and white."

Results and Interpretation

Many crosses were made between the normal type with complete seed coat and the defective types described above. Results are classed into three groups:

Type 1 × normal

(a) 3:1 ratio in F_2

Four crosses were made between types with normal seed coat and some of the types with defective seed coat. F_1 seeds were normal. F_2 results are shown in Table I:

TABLE I.—Results in F_2 showing segregation of normal and defective seed coats in 3:1 ratio

Cross No.	Parental Types	F_2 Type: Normal	F_2 Type: Defective	Total	χ^2	P
1	4-141×T124	20	4	24	0.8888	0.50-0.30
2	nao×T122	124	39	163	0.1003	0.80-0.70
3	nao×T173	22	7	29	0.0115	0.95-0.90
4	T144×T135	93	24	117	1.2564	0.30-0.20
	Total	259	74	333	1.3703	0.30-0.20

All χ^2 values in Table I are low and show good correspondence with the expected ratio. Therefore, one of the parental types of each cross contained one gene for defective seed coat.

(b) 15:1 ratio in F_2

Three crosses showed the same result as reported by Stewart and Wentz.[2] Both parental plants were normal for seed coat. F_1 seeds were normal and F_2 segregated into 15 normal to 1 defective. The results are given in Table II.

TABLE II.—Results in F_2 showing segregation of normal and defective seed coats in 15:1 ratio

Cross No.	Parental Types	F_2 Type: Normal	F_2 Type: Defective	Total	χ^2	P
1	Kingwa×Dunfield	152	9	161	0.1277	0.80-0.70
2	Sooty×Illini	225	19	244	0.9836	0.50-0.30
3	T69×Dunfield	58	4	62	0.0044	0.95-0.90
	Total	435	32	467	0.2969	0.70-0.50

All results show a satisfactory fit for a 15:1 ratio. One of the parents of each cross had black seed coat, while the other parent had yellow seed coat. Only one cross of this group was carried to F_3 (Table III).

TABLE III.—Results in F_3 of Sooty × Illini showing segregation for defective seed coats

Behavior in F_3	Observed	Expected	χ^2	P
Bred true for normal	29	24.5(7)	3.3264	0.50-0.30
Segregated in 15:1 ratio	13	14.0(4)		
Segregated in 3:1 ratio	9	14.0(4)		
Bred true for defective	5	3.5(1)		
Total	56	56.0		

The F_3 results are a good fit for the expected ratio of 7:4:4:1. All F_2 defective seeds should breed true. Of the F_2 normal 7 out of 16 F_2 should bred true for normal; 4 out of 16 should segregate into 15 normal to 1 defective and 4 in 16 should give a 3:1 ratio. This was substantiated by the results obtained in F_3. Since one of the parents in this group had black, normal seed coat, it is suggested that the other parent with yellow seed coats carried an inhibiting gene, I^{de}, which prevented the expression of de_1, necessary for defectiveness. Here, I^{de} might be equivalent to I as proposed by Stewart and Wentz,[2] or to one of the dominant genes in I, i^h, i^k, i series.

(c) 63:1 ratio in F_2

Three crosses showed a very small number of plants with defective seed coats. The results (Table IV) indicate that three recessive genes are necessary for defective seed coats. All parental types and F_1 plants were normal, while the F_2 population showed a very small number of defectives.

TABLE IV.—Results in F_2 showing segregation of normal and defective seed coats in 63:1 ratio

Cross No.	Parental Types	F_2 Type: Normal	Defective	Total	χ^2	P
1	T117A×P.I.135,624	51	2	53	1.7064	0.20 (nearly)
2	T124 × T135	56	2	58	1.4379	0.30-0.20
3	Mukden × Soysota	156	4	160	0.9270	0.50-0.30
	Total	263	8	271	3.4053	0.10-0.05

All results in Table IV show a good fit for 63:1 ratio. Since T135 had normal seed coat, and since it had been shown to contain one gene for defectiveness giving a 3:1 ratio when crossed with T144, the supposition is that it contains one inhibiting gene, I^{de}, to prevent the expression of the resessive gene for defectiveness. Therefore, we may assume that the genetic constitution of T135 is $I^{de}De_1de_2$, and that T124 has the constitution $i^{de}de_1De_2$, as de_1 expresses itself in the presence of i^{de}.

Only one cross, Mukden × Soysota, was carried to the F_3 generation. Fifty-six F_3 progenies were grown. Only one of them bred true for defectiveness, and this originally descended from an F_2 plant bearing defective seed coats. Ten progenies segregated into different ratios (Table V). If there is an inhibiting gene I^{de} involved for preventing the presence of defective seed coats as suggested above, this cross is then postulated to be $I^{de}de_1de_2$ (Mukden) and $i^{de}De_1De_2$ (Soysota). Therefore, a ratio of 63:1 should be obtained in F_2, and the F_3 population should segregate into a 48:9:6:1 ratio (Table V).

TABLE V.—Results in F_3 showing segregation of defective seed coats from the cross of Mukden × Soysota

Behavior in F_3	Observed	Expected	χ^2	P
Bred true for normal	45	41.76(48)		
Segregated in 15:1 ratio	6	7.83(9)	0.3450	0.98-0.95
Segregated in 3:1 ration	4	5.22(6)		
Bred true for defective	1	0.87(1)		
Total	56	55.68		

By the χ^2 test, the probability is high that there are two recessive genes, de_1 and de_2, responsible for defective seed coats. If the constitution is $i^{de}de_1de_2$, the seed coat will be normal. Only seed coats with constitution $i^{de}de_1de_2$, or $i^{de}de_1De_2$, or $i^{de}De_1de_2$ appear defective. The genetic constitutions of Type I soybeans are outlined in Table VI.

TABLE VI.—Genetic constitutions of various Type I soybeans and certain normal types involved in the present study

Type	Formula	Seedcoat character
T124	$i^{de}de_1De_2$	defective
T122	$i^{de}De_1de_2$	defective
T144	$i^{de}De_1de_2$	defective
4-141	$i^{de}De_1De_2$	normal
Dunfield	$I^{de}de_1De_2$	normal
Illini	$I^{de}de_1De_2$	normal
T117A	$I^{de}de_1de_2$	normal
Mukden	$I^{de}de_1de_2$	normal
nao	$i^{de}de_1de_2$	defective
T173	$I^{de}de_1de_2$	normal
T135	$I^{de}De_1de_2$	normal
Kingwa	$i^{de}De_1De_2$	normal
Sooty	$i^{de}De_1De_2$	normal
T69	$i^{de}De_1De_2$	normal
P.I.135,624	$i^{de}De_1De_2$	normal
Soysota	$i^{de}De_1De_2$	normal

Type II × normal

Three crosses were made and four series of F_2's were grown involving Type II and the normal. One of the parental types, T126 is a common one, and is typical of defective Type II. All F_1 seed coats showed more or less cracking, resembling that usually found in commercial varieties, as Chief and Dunfield (Figure 5, *C* and *D*). Whether this is partially dominant as considered by Nagai, can not now be determined. The cracking shown by F_1 seed coats is quite different from that shown by Type I or Type II. There is no large area of cracking or "network" formed in a definite region; hence the F_1 seed coats are considered to be normal. If these assumptions are made, all F_2 results (Table VII) can be explained logically.

TABLE VII.—Results in F_2 showing segregation of normal and defective seed coats in a 9:7 ratio

Cross No.	Parental Types	F_2 Type Normal	Defective	Total	χ^2	P
1	Blk Lincoln×T126	54	50	104	0.7911	0.50-0.30
2	T126×T117A	80	45	125	3.0517	0.10-0.05
3	T126×Chief(W)(1)	33	29	62	0.2304	0.70-0.50
4	T126×Chief(W)(2)	25	13	38	1.4050	0.30-0.20
	Total	192	137	329	0.5973	0.50-0.30

All results (Table VII) can be considered a satisfactory fit for the expected ratio of 9 normal to 7 defective seed coats. Therefore, two complementary genes are involved. The second cross is exceptional in that the parents differed in five pairs of genes i.e., $i^{de}De_1De_2de_3de_4 \times I^{de}de_1de_2De_3De_4$, yet no case of defective Type I was found. This may be due to difficulties in classification of Type I and Type II in complex combinations. However, the present conclusion differs from the results obtained by Nagai.[1] He assumed that defectiveness was partially dominant in F_1, since he obtained 9 defectives to 7 normals in F_2. In the present case, two dominant genes are considered complementary for normal, while the resessives alone or together, cause defectives. T126 contains both recessive genes, de_3 and de_4. Black Lincoln, T117A, and Chief have two complementary genes, De_3 and De_4, for normal. The F_2 generation was not grown. Genetic constitutions of parental types are given in Table VIII.

TABLE VIII.—Genetic constitution of various Type II soybeans and certain normal types involved in the present study

Type	Genetic Constitution	Seedcoat Character
T126	$i^{de}De_1De_2de_3de_4$	defective
Blk Lincoln	$i^{de}De_1De_2De_3De_4$	normal
T117A	$I^{de}de_1de_2De_3De_4$	normal
Chief	$i^{de}De_1De_2De_3De_4$	normal

(c) *Type I × Type II*

Only one cross was made between defective types I and II in order to determine whether they are due to the same genes (Table IX).

TABLE IX.—Results in F_2 showing segregation of normal and defective seed coats from the cross of T124 × T126

Seed Types	Observed	Expected	χ^2	P
Normal	50	40.5(27)	9.8974	0.02(nearly)
Heavy cracking or tendency to be like T126	35	31.5(21)		
Cracking like T124	5	13.5(9)		
Cracking like T124 and T126	6	10.5(7)		
Total	96	96.0		

The χ^2 value is so high that the p-value indicates that this formulation does not satisfactorily account for the distribution observed. However, we may make the following theoretical genetic analysis with the modification noted below:

(T124) $de_1De_2De_4 \times De_1de_3de_4$ (T126)

F_1 $De_1de_1De_3de_3De_4de_4$

F_2			
27	$De_1De_3De_4$	normal	
9	$De_1De_3de_4$	like	T126
9	$De_1de_3De_4$	like	T126
9	$De_1de_3de_4$	like	T124
3	$De_1de_3de_4$	like	T126
3	$de_1De_3De_4$	like	T126 and T124
3	$de_1De_3de_4$	like	T126 and T124
1	$de_1de_3de_4$	like	T126 and T124

Therefore, the summary from the above analysis of defective genes is as follows:

Seed Types	*Proportional Number expected*
Normal	27
like-T126	21
like T124	9
like T126 and T124	7
Total	64

In Table IX, the first class is too high, while the last two are too low. These deviations from expectation may be due to the fact that there are a number of genes involved and that there is genic interaction for defectiveness.

As shown in Figure 4, only the seedcoats in the first group of F_2's with the constitution $De_1De_3De_4$, are normal. The second group occurring in the proportion of 21 out of 64 is supposed to be like T126. Only $De_1de_3de_4$ showed nearly the same appearance as the original parent (T126), while $De_1De_3de_4$ and $De_1de_3De_4$ are more or less affected by the presence of dominant De_1. A number of seeds in the latter class showed a distinct area on the sides of the seeds, where the network is located as on T126. There was a very remarkable tendency to form cracking on the seed surface, but the outer layers of the seed coat were still present, as shown in the classes, $De_1De_3de_4$, $de_1De_3de_4$, and $de_1de_3De_4$. In the presence of both dominant genes, De_3 and De_4, in the third group of $de_1De_3De_4$, it is assumed that De_3 and De_4 inhibit the expression of de_1; hence, in the F_2 progeny no real defective seed coats of Type I occurred. However, splitting lines do occur on the surface of the seed coats. Either De_3 or De_4 may express its effect individually, so that no class showed exactly the characteristics of the combination of both types. In the fourth group, there was some tendency to show cracking on the seed coat, as in $De_1De_3de_4$. Furthermore, there were some short splitting lines here and there on the seed coats. The pure recessive type was found in a

very small proportion. These facts coincide with the assumption that genes for defective Type I and II differ both in their expression on the seed coats and in their behavior in inheritance.

Summary

A genetic analysis was made of defective seed coat in soybeans. Two types, I and II, of defectiveness were recognized and described. Type I is mainly determined by two recessive genes, de_1 and de_2, accompanied with i^{de} for no inhibiting effect on defectiveness, resulting in three kinds of phenotypic ratios, as 3:1, 15:1, and 63:1, when crossed with normal. Type II is probably produced by two complementary genes, de_3 and de_4. Various degrees of defectiveness were detected in the progeny derived from the cross between Types I and II. This is believed to furnish evidence that each of these types is affected by a different group of genes for defectiveness.

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FACTORS OF EVOLUTION

A Review

IN 1896 the American biologist J. M. Baldwin briefly called attention to "a new factor in evolution." This was discussed by Lloyd Morgan and Henry Fairfield Osborn, among others, and then presented at length in a book by Baldwin.[1] Explicit references to Baldwin's "new factor" in later studies in English or western European languages are few and brief. The factor has not been wholly ignored nor has its reality been seriously questioned. The impression is given that students did not forget the "new factor" but relegated it to a role so subsidiary as to require little comment. Around 1935, however, the Soviet biologists Kirpichnikov and Lukin independently "rediscovered" Baldwin's evolutionary factor, an identity and priority freely recognized in the Russian publications. In the Soviet Union this became the basis for what was, by intention, "the new trend in biology" and a new school of evolutionary theory. Since the "rediscovery," leaders in this have been G. F. Gause, working experimentally mainly on populations of *Paramecium*, and I. I. Schmalhausen, working on descriptive and experimental embryology, especially of the fowl. The original literature, in Ukrainian and in Russian, has not been generally noticed by students outside the Soviet Union, but a summary by Gause appeared two years ago in English,[2] and now we have an English version of a full-length presentation by Schmalhausen.

The evolutionary factor in question has been variously called "organic selection," "harmonious selection," "coincident selection," or "stabilizing selection," the last being the term favored by Schmalhausen, as in the subtitle of this book. In simplest presentation, the idea is that adaptive modifications, produced in individuals by environmental conditions, may become fixed and hereditary by the natural selection of genetic mutations tending to produce the same effects. In this way acquired characters might, indeed, be replaced by hereditary characters, by a process consistent with modern genetical theory. Followers of his theory might thus accept the evidence of the neo-Lamarckians or of Lysenko at face value, and still accept also the full findings of orthodox genetics.

The popularity and intense cultivation of this solution occurred in the Soviet Union in the period of struggle between the Mendel-Morganists and the Michurinists. Schmalhausen is at pains to label his views as truly Darwinian, merely a development of the ideas of the impeccable master, and hence dialectic-

**Factors of Evolution.* The Theory of Stabilizing Selection. I. I. SCHMALHAUSEN. Blakiston, Philadelphia and Toronto. Translated from the Russian by I. Dordick. Edited and with a foreword by Th. Dobzhansky. xiv + 327 pp. 42 figs. 1949. $6.00.

ally correct. He also attacks the "neo-Darwinians" (who are mainly the capitalistic geneticists). This is, however, all too plainly lip service. It is clear enough that his whole outlook on evolution is neo-Darwinian, in the ideologically damned sense, and he flatly opposes Michurinism, without, of course, mentioning Michurin or Lysenko by name. The hope for a valid compromise between the conflicting parties therefore has come to an end, and no more studies of this sort can be expected from the Soviet Union during the ascendency of Lysenko under the dictatorship. After his book was published in Russian, Schmalhausen soon became a *former* member of the Academy and *ex*-Director of the Institute of Evolutionary Morphology. "The new trend in biology" (Gause's expression) has ended in its homeland, but the scientists of other countries can fortunately evaluate its findings free of political pressure.

In this book it is Schmalhausen's aim to examine the interplay of selection and heredity in terms of the "norm of reaction, embracing both the structure and function of the organism during its entire ontogeny." Developmental processes occur by interaction of heredity and environment and may be (a) dependent, responding directly to the presence and intensity of environmental stimuli, (b) autoregulative, depending on an environmental threshold for their activation but thereafter proceeding independently to an end, or (c) independent or autonomous, genetically determined without regard to environmental conditions. A role of stabilizing selection is that it tends, in the course of evolution, to transfer developmental processes progressively from the first to the last of these categories. Or, put in another way, organisms originally capable of a wide variety of acquired modifications in response to environmental factors may by stabilizing selection have these responses restricted to a single morphogenetic sequence which will, finally, appear even in the absence of the corresponding environmental stimulus. Baboons may be supposed once to have had a labile reaction norm, with callosities appearing only where and when there was repeated contact with the ground and in proportion to the duration and roughness of such contact. Stabilizing selection then favored a new norm in which callosities appear autonomously, by heredity, even before any contact occurs.

In this sense, "stabilizing selection" is most strictly defined and is narrowly equivalent to the "organic selection" of Baldwin and others, down to Gause. Schmalhausen, however, also uses "stabilizing selection" in at least two other senses, without clearly explicit distinction. In some passages it seems merely to mean selection in favor of wild type, or of an established norm—what is sometimes called "centripetal selection." Elsewhere, and more frequently, it means selection favoring and, in a sense, producing "internal regulating mechanisms which counteract the harmful influences of the external environment" or of disadvantageous mutations and other disintegrating factors. Among the mechanisms of this sort discussed at some length are diploidy, dominance of wild type, balanced genetic systems, determination of morphogenesis by the genome as a whole, wide range of normal tissue reactivity, and complexity of morphogenetic correlations.

Stabilizing selection in the narrowest sense, or "organic selection" of genotypes coincidental with adaptive induced modifications, is doubtless a real phenomenon. It seems to have been established experimentally by Gause and to be the most probable explanation of some observed cases of adaptation. It is also of considerable interest in its bearing on some of the test cases of neo-Lamarckism. There is, however, room for question whether it merits such extreme emphasis, aside from the ideological struggle which evidently stimulated some of these studies. That it is really of relatively minor importance is suggested by the fact that when Schmalhausen is discussing broader problems and general historical aspects of evolution he hardly ever uses "stabilizing selection" in this sense, but almost exclusively in the sense of the development of regulating mechanisms.

In this latter sense, only the term is new to the non-Russian developers of the modern so-called neo-Darwinian or, better, synthetic theory evolution. Schmalhausen's discussion of this topic is, nevertheless, a profound and able presentation and a major contribution to this theory from a distinct and fresh point of view. This is the core of the largest of the four sections of his book (Chapter 3, 108 pages). The first two chapters (of 46 and 92 pages, respectively) are constituted, in the main, by a review of the topics of variability and of population genetics, which Schmalhausen considers less original but which also makes a great and welcome contribution because of its insistence on the developmental point of view, on reaction systems in whole ontogenies and not on developed characters or mutations and frequencies of single genes. Here Dobzhansky seems justified in considering the book as an essential advance in the synthetic view of evolution, "an important missing link" in the modern theory. The synthesis has already widely involved genetics, systematics, biogeography, paleontology, comparative morphology, and several other disciplines and now has available for its fabric also data from comparative and experimental embryology.[3, 4]

Dobzhansky notes[3,4] that among the life sciences "only physiology and biochemistry still remain largely unaffected by evolutionary ideas." That this is, in general, true bespeaks the immaturity of these recently ascendant branches of biology. There are, however, happy omens that the lag is temporary.

The last and briefest section of the book (Chapter 4, 38 pages) is devoted to rates of evolution and to a summary of the history of life from the author's special point of view. Stimulating concepts and flashes of insight are not lacking, but to the reviewer this chapter is disappointing. Point by point criticism is not called for, but on these topics the author, here outside his immediate field, occasionally betrays inadequate grasp of the facts and becomes diffuse and confused.

The book as a whole is exceptionally difficult. This arises in part from the complexity of the subjects treated, for few can bring to the reading such broad knowledge and such comprehension of abstruse details as are possessed by the author. The difficulties are, however, partly semantic and organizational and might have been avoidable either in the original or in the translation. A few special terms of the Russian school of morphogenesis are explained in the foreword, but others strew the rough path of the English reader. The translator may be to blame for some infelicities, such as a sentence starting, "Indeed the constancy of the average numbers of individuals of species at a given time is constant. . ." Sentences more or less like the following are frequent: "The evolution of ontogenesis generally consists of a progressive autonomization of the most important morphogenetic processes which are associated with a regulative organization of the inductive systems." This seems to be proper English and is probably "logically impeccable" (these words are applied by Dobzhansky to Schmalhausen and by Schmalhausen to Darwin)—but the meaning does not leap to the mind! Although some cuts were made by the editor to eliminate repetition, there is still considerable repetition, understandable in a book written under war conditions (the manuscript was completed about 1942). The author has noted that contradictions occur, but maintains that these are objective contradictions in evolutionary processes. The reviewer suspects subjective contradictions in some of the, to him, more confusing passages.

Whatever small faults exist, the book is an important one, essential reading for students of evolution. Dordick and Dobzhansky have painstakingly made available to non-Russians a new world of stimulating ideas, ideas more than ever to be cherished and evaluated now that fair consideration cannot be given them in their land of origin.

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Human Isolate Essay Contest

(*Continued from page* 312)

their intention to participate, and outline their general plan of approaching the problem together with revelant qualifications. If in the estimation of the judges, the approach seems unlikely to yield significant results, or the applicant appears to lack qualifications necessary for successful competition, the Association reserves the right to advise that the application be withdrawn. The Association reserves the right to close the contest to further applications at any time.

Essays must be mailed by midnight February 28, 1951. The winner will be announced in the August, 1951, issue of the Journal of Heredity.

If in the estimation of a majority of the judges none of the essays submitted is of a quality to justify the award of first or second prize, the Association reserves the right not to award either or both of these prizes.

Contestants will submit standard-size double-spaced typewritten copy—original and two carbons. The essays should run from 100 to 200 double-spaced pages (25,000 to 50,000 words), though no minimum length is specified. The contestants will retain copyrights to their essays. If, as expected, the Journal of Heredity should publish the winning essay, copyright arrangements will be made with the author.

Competition is open to any qualified participant, except persons employed by or closely related to employees, officers, or committee members of the American Genetic Association.

THE DISINTEGRATION OF CIVILIZATION

S. C. REED
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THOSE interested in human genetics and eugenics can profit from reading Professor Arnold J. Toynbee's *A Study of History*. It demonstrates that civilizations do not die from attacks by external enemies but are quite clearly the victims of involuntary self destruction. Of the 21 civilizations that Toynbee lists as having been born alive and grown to maturity, all are now dead or in decline except the Western. Even that seems to have entered upon its "time of troubles." Can our Western Civilization learn enough about the "suicides" of the others to escape the same fate?

Suicide may be a poor word for the breakdown of a civilization, for it implies a conscious motivation toward self-destruction which a civilization would not be expected to possess. The word *does* suggest a biological phenomenon, and emphasizes that civilizations are composed of human beings. Consequently one would expect an extensive consideration of the *biological aspects* of the disintegration of civilizations in a work already embracing six volumes. It is therefore most disappointing to find that biological factors are dismissed with a few quotations in the original Greek!

The possibility that the phenomenon we call civilization is in the long run essentially dysgenic seems to be personally repugnant to Professor Toynbee. Perhaps this explains his dismissal of possible genetic degeneration of a society without adequate consideration. He insists that the biological heritage of the decadent children is the same as that of the pioneers. Perhaps it is. *The biologist would like to him prove it.*

Had Professor Toynbee mentioned the possible adverse effects of differential birth rates upon the mental ability of subsequent generations of a population, and accepted this principle as having any validity, it seems probable that he would have considered the results a product of social decline and not a cause of it. Evolution tells us that the hen and the egg evolved together! The Professor makes the point that the fundamental failure of a civilization is the decrease in the creative power of the minority. He cannot seem to take the next step and admit that when civilization is in flower highly creative individuals, as a group, will not and do not produce as many children as the uncreative. If the ability of the creative minority is in *any way* genetic, it would be reasonable, though not necessarily correct, to assume a gradual decrease in the mental and creative ability of a civilized population. If there is *any* consistent and significant drop in mental ability with each succeed-

TABLE I.—The disintegration of civilizations. Data taken from the table compiled by Dr. Myers and reprinted as Table V of the Somervell abridgement of Toynbee's, "A Study of History."

Civilization	Approx. Beginning	Time of Troubles Through Universal Peace	Beginning to Time Of Troubles (Years)	Time of Troubles Through Universal Peace (Years)
Egyptiac	4000 B.C.	2424-1660 B.C.	1576	764
Sumeric	3500 B.C.	2677-1905 B.C.	823	772
Sinic	1500 B.C.	634 B.C.-A.D. 172	866	806
Syriac	1100 B.C.	937- 332 B.C.	163	605
Hellenic	1100 B.C.	431 B.C.-A.D. 378	669	809
Far Eastern (Mongol and Manchu)	A.D. 500	A.D. 878-1853	378	975
Far Eastern (Japanese)	A.D. 500	A.D. 1185-1863	685	678
Hindu	A.D. 800	A.D. 1175-1948(?)	375	773(?)
Orthodox Christian (Ottoman Empire)	A.D. 700	A.D. 977-1768	277	791
Orthodox Christian (Russian Offshoot)	A.D. 900	A.D. 1075-1881	175	806
			Av. = 599 years	Av. = 778 years

ing generation, collapse must come eventually and inevitably.

Dr. Myers has compiled a table which is reprinted in the Somervell abridgement of Toynbee's "Study." This table gives the dates for the 21 civilizations mentioned above where definite endpoints can be ascertained. The ten civilizations for which sufficient information is available are given in detail in Table I. The present writer has carried out the simple determination of the number of years from the approximate beginning of the civilization until the start of the "time of troubles." A second determination gives the span of years from the start of the "time of troubles" until the end of the period of "universal peace," a point which is considered a good marker for the death agonies of the civilization. If the reader wishes to add the last two columns of Table I he may obtain the total life span of each civilization.

Consideration of the last two columns of the table provides two plausible generalizations. The first is that successive civilizations arrive at their "time of troubles" sooner. Perhaps this means that they mature faster, nourished by the social legacies from their predecessors. The second observation is that the length of time included from the beginning of the "time of troubles" until the end of the period of "universal peace" is remarkably constant. All of these latter values are within plus or minus 200 years of the average of 778 years.

We might assume that once the creative minority becomes established it should thereafter perpetuate itself indefinitely. The observation that it disintegrates about 800 years later, and with some precision, indicates a genetic self-limitation of which a fairly definite number of reproductive generations is a function.

The average of 778 years during which the average civilization loses ground is the equivalent of only 31 generations, allowing 25 years per reproductive generation. If the mental ability of the population dropped consistently for as many as 30 generations, the average drop per generation would not have to be at all great to guarantee collapse of the civilization.

It may be useful to remind the reader that the negative correlation between educational accomplishments and the production of children has been firmly established. The evidence is not unequivocal as to whether this is purely a social phenomenon or is also in part genetic. The data needed to answer this question could be obtained with a modest investment of time and resources. The answer should allow us to predict whether our Western Civilization may be expected to disintegrate according to schedule.

The challenge to the geneticist is to answer the question posed before or during the Hellenic Civilization, "Are differential birth rates truly dysgenic?" It is possible that differential birth rates in Sweden today are actually eugenic. If such is the case, and if other segments of Western Civilization follow the Swedish example, one may speculate about this alternative to suicide with at least a trace of optimism.

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"FREE" SCIENCE IN AMERICA

EDITOR Cook, in the July JOURNAL OF HEREDITY, made a valuable and timely contribution to a troublous and critical situation in the field of genetics. However, as Mr. Cook has pointed out, the trouble goes deeper than "the inheritance of acquired characters," or the whole field of genetics, deeper even than the entire field of science.

Possibly the worst feature of the matter is the hysterical condition of the general public in the United States of America today—a condition which, unfortunately, *may* be duplicated in Russia. Here are two great peoples at violent odds; one claiming to stand for democracy, the other for communism, though the behavior of both is a travesty on the names. The controversy has reached such a pass that reason and logic have been abandoned. In America, no more opprobious epithet can be applied than "communist," and the same attitude exists among the Russians in regard to "capitalist democrats." The situation has degenerated to such a point that even our diplomats at times act and talk more like ten-year old ruffians

than men representing great nations. And as for the so-called newspapers of this country, they have, in general, disgraced themselves beyond redemption. It is virtually impossible any longer to get the truth, or even the near truth concerning Russia from the public prints. Ballyhoo and abuse of communism is the best possible technique for furtherance of the advertising racket.

Worst of all, so far as the genetics controversy is concerned the man on the street believes in the inheritance of acquired characters. If he gave the question a moment's reflection, he would not, but the man on the street does not reflect. He is controlled by his prejudices and the propaganda of his immediate environment. His thinking is mostly of the wistful variety. Most clerics accept this vagary, as do many social workers and teachers. In fact, were it not for the popular hysteria against Russia today, they all would be trailing after Lysenko like rats after the Pied Piper.

Concerning this I know whereof I speak. A number of years as lecturer on genetics, heredity and allied subjects has made possible for me a valid estimate of audiences—"intelligent" audiences, as well as the mediocre. It must not be overlooked that the public is 99.9 per cent uninformed upon this very important subject. It is taught not at all in the high schools, and very little even in the colleges and universities. Though the most important subject in the curriculum, it is never "required." Set over against this the incessant pulpiteering on the other side—i.e. in favor of inheritance of acquired characters—and we have a not very beautiful picture.

Permit me to illustrate. Not long since, I was sitting in my study, reading the JOURNAL OF HEREDITY. A maid came in to dust the furniture, stopped in front of me, eying the bold-typed "HEREDITY" on the magazine cover. She said, "Do you believe in heredity?" I asked her to have a seat and told her that heredity was not a matter of belief any more than was sunshine or digestion; then took twenty minutes to educate her. Being a rather intelligent woman, she quickly grasped the significance of the subject but when I had finished, heaved a sigh, with, "Oh, I'm sorry that is true. I don't want my daughter to be as fat as I am."

Recently, in a town of 2,000 population, a high school teacher told his students something of heredity. This fact reached one of the local clerics. Result: the local ministerial association waited upon the school superintendent and exacted a promise that the subject of heredity should not again be mentioned to the students. Now the tragic part of this is the fact that both clercis and superintendent not only had no information upon the question, but probably both were too stupid to understand the subject of genetics in any event. Yet these are the intellectual leaders of the community.

At the time of the Scopes trial, I was living in a city of thirty thousand. The local Rotary Club asked me to discuss evolution, but the pressure of the local clerics was sufficient to get the club to withdraw the invitation. At the same time, the local paper secured assurance from the superintendent of schools that "evolution" is not mentioned anywhere at any time in the schools—except that "the word unfortunately occurs in the dictionary." When I was on the faculty of the State College, the president of the school received a request from the program committee of the state teachers association asking me to lecture on evolution at its annual meeting. Before the invitation could reach me, a protest from a group of clerics killed it.

Within the last year, a cleric in a city of forty thousand was asked to talk to the local chapter of the American Association of University Women. He selected as his topic, evolution, about which he knows nothing, and regaled this audience of seventy or eighty university graduates with arguments against evolution, assuring them that it is no longer believed in even by scientists. Most astounding, not one of the audience so much as questioned him. The local paper reported his talk, but refused a signed letter correcting him.

These incidents did not occur in the Bible Belt. They occurred in the enlightened state of Washington, where less than fifteen percent of the population are members of any church.

The teaching of biological science in the secondary schools of the country today is largely a joke. The average student, even of the so-called biology course, would not know what you were talking about if the inheritance of acquired characters were mentioned, and if the question were put to the average superintendent, his reaction would be about the same as that of the former Sultan of Turkey, who had employed some foreign engineers to establish a lighting system for Constantinople. When they mentioned to him the installation of a number of dynamos, the Sultan abruptly stopped them and cancelled the whole project, as he did not intend to have anything as dangerous as dynamos in his domain.

All of which raises the question as to whether we are much further advanced than Russia in our thinking. We must not forget that approximately ten percent of our population we regard as too dark to educate. Then some fifteen percent of the remainder are "educated" in institutions which are a travesty upon the name of education. The prime purpose of these so-called schools is indoctrination of the pupils' minds with dogma rather than leading them out. The products of these schools are able to prevent the City College of the nation's chief city from employing as a lecturer the world's leading philosopher. They are also able to dictate to the school board of this same city what magazines it shall furnish or not furnish for its pupils. Let

us be sufficiently charitable to hope the Russian schools are not thus handicapped.

Editor Cook has called attention to the deplorable and disgraceful truckling of certain Russian scientists. His criticism is very much in order, and not too severe. However, what can we say of our own scientific charlatans? What of Lecomte du Noüy's illogical vagaries bearing the stamp of approval of *one of our prominent scientists?* A more shameful case of scientific prostitution would be difficult to find, yet it was touted to high heaven by the popular prints, e.g. *Readers Digest.* Yet the latter magazine refused to print any criticism of it as it did of "Man Does Not Stand Alone," another of the same type.

In fact, if anyone is so naive as to think freedom of thought exists in this country, just let him try to secure publications of anything critical of any religious denomination, the D. A. R., the American Legion, or any one of a number of other of our idols or sacred cows, in any journal which derives a significant income from national advertising. Is it not well recognized that the so-called newspapers are noted more for the news they suppress than that which they publish?

On page 193, Editor Cook calls attention to the reported intention of the Soviets to rewrite the encyclopedia to meet the ideology of the party in power—disgraceful enough, but we in the land of free and objective thought have already beaten the Muscovites to it! We have revised our leading encyclopedia to meet the desires of the Roman hierarchy (See "Lies and Fallacies of the Encyclopedia Britannica," by Joseph McCabe, Haldeman-Julius Publications, Girard, Kans.); a more disgraceful prostitution of education can scarcely be imagined.

Russia is criticized for control of its scientific men. Read the vigorous official protest of the American Association for Advancement of Science against science in the United States of America being controlled by a military cabal (*Science* 110:177). Or see the article in *Harpers* (197-61) by Attorney General Arnold warning *ALL* government employees from even associating with anyone whose thinking is at all unconventional.

I hold no brief for Russia. It may be entitled to all the criticism we have heaped upon it, though I doubt it; but with our own linen as dirty as it is, our strictures are scarcely in good taste.

Mr. Cook refers (p. 196) to alleged efforts of the United States State Department to effect cultural and scientific exchanges with U.S.S.R. In this connection, one should read "The Truth About American Diplomats" by Annabelle Bucar, Supplement to *New Times*, No. 11, March 9, 1949.

IRA D. CARDIFF

Yakima, Washington

Everything Dr. Cardiff says is quite true. Our school system is a mess. Our diplomacy too often stinks. Some of our scientists who ought to know better endorse the darndest books. One could go on *ad infinitum*. But as pointed out in the article which Dr. Cardiff criticises, things remain for all that *less worse* here. To say that is not mere chauvinism. "The Central Committee of the Party examined my report and approved it" has not yet happened here. Whether it ever will is a question we should not dismiss too casually. But of right now, we still have a slight edge. Our numerous bloomers in the field of the application, orientation, and interpretation of science are inspirational rather than by order of "The Party." That makes them sometimes funny, and always a little less sinister.

R. C.

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PROFESSOR VANDEL COMMENTS

(*See page* 313)

Monsieur le Professeur Th. Dobzhansky a eu l'aimable attention de me soumettre, avant sa publication, l'analyse qu'il avait rédigée de mon livre: "L'Homme et l'Evolution." Encore que cette critique soit parfaitement impartiale, il me semble néammoins utile d'ajouter quelques remarques au compte-rendu du Prof. Dobzhansky. Le point de vue de plusieurs biologistes français est, en effet, aujourd'hui trés éloigné des positions adoptées par la plupart des savants anglo-saxons.

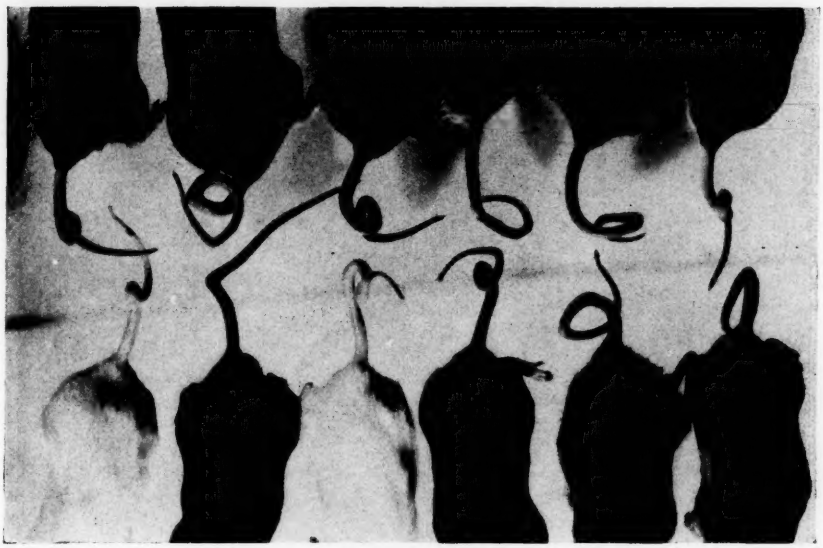
I) La grande majorité des biologistes anglo-saxons admet que la sélection naturelle joue un rôle tout puissant, et même exclusif, dans l'évolution. C'est là le résultat du retentissement extraordinairement profond que l'oeuvre de Charles Darwin a eu sur les biologistes de langue anglaise. Il n'est point question de minimiser la valeur des admirables travaux de R. A. Fisher, de J. Huxley, de Sewall Wright, de Th. Dobzhansky, et de tant d'autres biologistes anglo-saxons. Il est inutile de rappeler les données extrêment intéressantes que leurs recherches ont apportées aux problèmes des populations, à la genèse des variétés, des races, voire des espèces. Mais, j'ai le sentiment que le rôle de la sélection naturelle—si évident dans sa fonction d'élimination des inaptes—n'a jamais été clairement démontré dans la création de *nouveaux types d'organisation*. Il m'apparait que de petites variations de hasard ne sauraient avoir aucune utilité réelle pour l'espèce, et ne pourraient donc point donner prise à la sélection naturelle. L'"utilitarisme" outrancier des darwiniens et des néo-

(*Continued on page* 334)

HEREDITARY LOOP-TAIL IN THE HOUSE MOUSE

Accompanied by Imperforate Vagina and with Lethal Craniorachischisis When Homozygous

L. C. STRONG AND W. F. HOLLANDER*
Yale University School of Medicine



PIGTAIL MICE
Figure 6

Variations in tail-looping in a substrain of Strong's "A" strain. Loop-tailed mice do not breed pure. In outcrosses with straight-tailed, a majority of the progeny had looped tails.

IN the eighty-fourth generation of brother-sister inbreeding, mice of Strong's "A" strain[8] produced several young with tails strikingly looped. Anatomical abnormalities in this strain had previously been limited to harelip and cleft palate, and a few instances of ocular lesions.

Since the parents of the loop-tailed mice were normal, it seemed likely that recessive segregation had occurred. However, when the loop-tailed mice were mated together they failed to breed true as expected; that is, some of their offspring were normal. Consequently a more detailed study has been undertaken in order to determine the inheritance of the condition.

Preliminary outcrosses were made with normal mice of several other strains. In each cross some of the hybrids were loop-tailed, suggesting the presence of a dominant factor. Since mice with normal tails greatly exceeded those having looped tails, some further complication appeared to be involved.

It was noted that the degree of contortion of the tail varied widely, even among

*This study has been made possible by grants from The Anna Fuller Fund, The Jane Coffin Childs Memorial Fund for Medical Research, and The Committee on Growth of the National Research Council acting for The American Cancer Society.

STRAIGHT-TAILED, "LOOPED," AND DEFECTIVE EMBRYOS

Figure 7

Litter showing two straight-tails, two looped-tails, and two defective embryos. The syndrome of defects, including tail loops, "wobbly behavior," and defects of the spine and head, appear to be conditioned by a single dominant gene.

litter mates. Some extreme "pretzel twists" occurred, along with minor angular crooks or curves. At the same time a behavior anomaly became recognized in all the loop-tailed mice (of any degree): a nervous rocking or wobbling of the head. This condition also varied to some extent, tending to mild choreic activity in a few cases. It differs from the "waltzing" and "shaker" traits previously studied in the mouse, and furthermore it is not associated with deafness.

A few of the hybrid mice with normal tails were "wobbly." The question next demanding an answer was whether the tail anomaly and the behavior anomaly were genetically related or independent. Further outcrossing of these mice with unrelated normals demonstrated close genetic similarity — the straight-tailed "wobbly" mice produced some loop-tailed hybrids (see Table I). There are marked discrepancies in the proportions of the several types in the different matings which are difficult to explain. Many potentially "wobbly" straight-tailed offspring may have been discarded along with wholly normal young early in life, so that one must use the frequencies in this class as minimums only. However,

we have seen no evidence that enough "wobbly" offspring are produced to equalize the ratio of abnormal to normal in these matings.

The best hypothesis to account for the above data seems to be that a single dominant gene with incomplete penetrance and expressivity governs the loop-tailed and wobbly traits; probably modifying genes in certain crosses are effective in reducing the incidence of tail abnormality.

Still another anomaly became apparent in some of the loop-tailed and wobbly females, namely, imperforate vagina. This condition is obvious in consequence of the perineal swelling, due to gradual acumulation of fluid in the uterus. The frequency of the condition has varied considerably in different families, but averages nearly a third of all loop-tailed or wobbly females. Attempts to establish a permanent vaginal orifice by incision and temporary plugging have been carried out in numerous affected females by Dr. C. A. Pfeiffer, with occasional success. Two such operated animals gave birth to litters.

The next question to be tested was whether the postulated dominant gene would give the same effects when homozygous. For this test various matings of loop-tailed and wobbly mice were made. No new types of viable offspring resulted, but the litter size (Table I) was so markedly reduced in almost all these matings that prenatal mortality was suspected. Pregnant females from all the types of matings were therefore killed for comparative examination of fetuses. In practically every case, if the parents were both abnormal, some of the fetuses were monsters, having craniorachischisis (Table II). Although generally alive even in the last day or two of pregnancy, these monsters have never survived birth; apparently they are always eaten (more or less completely) by the mother. Since such embryos have not been found in any other mating, and since their frequency in this type of mating approximates 25 percent, it appears that they are the homozygous class. The postulated gene when heterozygous produces variable tail looping, wobbly behavior, and sometimes imperforate vagina. When homozygous it has much more drastic effects, and is lethal at birth.

Surprisingly little variability has been noted in the monstrous embryos. They are readily recognized with the naked eye after about the tenth day of pregnancy. The neural tissue, failing to form a tube, develops as a herniated cranial mass with two broad tracts down the back, with a narrow groove between them. In the last two or three days of pregnancy the neural tracts have generally fractured across the lumbar region, with considerable hemorrhage into the amniotic fluid.

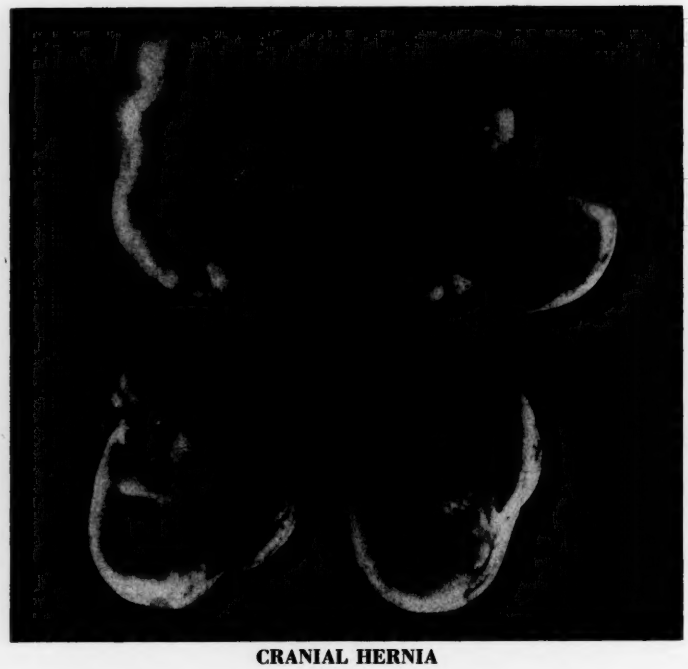
With the craniorachischisis the skin has the appearance of being neatly excised over the whole dorsal zone, the line running from the frontal region just over the eye and ear, along the proximal ends of the ribs, and ending about midway down the short reflexed tail. The urogenital and anal openings seem entirely normal. In a fair proportion of cases there is a small umbilical hernia, and in two cases there has been complete failure of ventral closure, with heart, lungs, liver, and intestine exposed. In gross size the monstrous embryos have generally been slightly smaller than their normal or loop-tailed siblings, especially near the end of pregnancy.

Only one fetus has been difficult to classify: its only abnormality is cranial hernia. The mother had two other embryos with typical craniorachischisis (Figure 8). In Table II the problematic specimen has been classed with craniorachischisis.

In view of the drastic abnormalities of the spinal region in the monstrous fetuses, we considered the possibility of occult spina bifida in the heterozygous loop-tailed adults. Examination of X-ray photographs of a group picked at random revealed no sign of spinal abnormalities above the tail area.

Discussion

Numerous reports of tail abnormalities in the house mouse are on record.



CRANIAL HERNIA

Figure 8

Litter of four, in which two at top have typical craniorachischisis and one has a cranial hernia.

Our mutant type resembles a type named "pigtail" of Crew and Auerbach[2], but it cannot be identical since the latter is recessive. It should be noted that spina bifida was found in some of the mice of the pigtail strain.

Loop-tail is also suggestively similar to the type known as "kinky"; the latter is dominant, lethal when homozygous, and the kinky adults sometimes show choreic head movements. However, unlike loop-tailed mice, kinky mice are frequently deaf, and the homozygous embryos probably do not show craniorachischisis, for no report of such abnormalities has been made.[6]

Since "kinky" and certain other inherited tail abnormalities form a remarkable pseudo-allelic group of genetic traits involving complex problems in symbolization, we have given loop-tailed specimens to Dr. L. C. Dunn of Columbia University for further special study in that connection.* Dunn and associates[4,3] have reported spina bifida as a fairly common feature of several of the genetic

*Loop has been tested for allelism and linkage with the *T* (Brachy) locus by L. C. Dunn who sends the following report: "Loop ♂ crossed with ♀♀ having two *t* alleles, either t^3t^3 or t^0t^3 gives (in 34 progeny) only Loop or normal, never tailless. Tt^3 and Tt^0 are tailless; hence Loop is probably not a recurrence of *T*. Loop crossed with Brachy gives Brachy, Loop, normal and Brachy Loop (7:5:17:4). Brachy Loop animals were first tested by t^3t^3 to confirm presence of *T*: and then test-crossed to normals (Bagg albinos) and produced 52 normal, 42 Loop, 42 Brachy and 13 Brachy Loop. Of the latter, 5 had large spina bifida aperta and 2 were extreme cases of rachischisis totalis; all of these were born dead. It is apparent that *Lp* and *T* segregate independently and that the double heterozygote is often abnormal and suffers high mortality at or before birth. Loop interacts similarly with Fused, the double heterozygotes often showing spina bifida, but since Fused is very near to Brachy, no linkage tests of Fused with Loop were run."

VARIATIONS IN "PIG TAILS"
Figure 9

The extent of deflection of twist from normal straightness ranges all the way from bends and twists to tight spirals.

combination types involving tail anomalies. A genetic type more approaching craniorachischisis was reported by Bonnevie[1] but is apparently no longer in existence.

Cranial hernia occurs very rarely among fetuses of ordinary strains of mice. One in a thousand is probably about its highest frequency (we have observed three cases among several thousand embryos). But among the descendants of X-ray treated mice, Snell[7] has found high frequencies of cranial hernia and other anomalies of the central nervous system. Translocation of a chromosome appears to furnish the hereditary basis of these disturbances, and the abnormal fetuses are produced even in outcrosses. The hereditary basis of the craniorachischisis here reported may

TABLE I. Young born from matings involving loop-tailed and "wobbly" mice. The first four rows of matings are outcrosses to unrelated normal mice. The term "wobbly" refers to the mice without loop-tail, but not normal.

Litters	Dam	Sire	Offspring			Av. Litter size
			Normal*	Wobbly*	Loop	
36	normal	loop	176	15	69	7.2
16	"	wobbly	82	1	24	6.7
14	loop	normal	56	4	32	6.6
4	wobbly	"	29	11	1	7.0
39	loop	loop	64	11	62†	3.6
1	"	wobbly	4		3	
3	wobbly	loop	8		3	3.6
10	"	wobbly	18	3	1	2.2

* Some classed as normal were killed before behavior was clear.
† Also 3 unclassifiable (destroyed by dam).

TABLE II. Types of fetuses from matings involving loop-tailed and "wobbly" mice. The first four rows of matings are outcrosses to unrelated mice. Resorbing embryos are too decomposed to classify.

No. of dams	Dam	Sire	Fetuses				Av. no. of Fetuses
			Nor.	Loop	Cranio-rach	Resorbing (dead)	
18	normal	loop	85	39	0	19	8.0
6	"	wobbly	34	12	0	9	9.2
15	loop	normal	73	31	0	8	7.5
1	wobbly	"	5	2	0	0	
17	loop	loop	42	31	27	8	6.4
1	"	wobbly	3	0	5	0	
3	wobbly	loop	5	5	8	2	6.7
3	"	wobbly	17	4	4	1	8.7

possibly be a chromosome abnormality rather than a simple gene change, but it seems decidedly different from translocation transmission.

Spina bifida and related neural defects in man are well known and commonly are referred to as "accidental developmental arrests." However, considerable evidence for hereditary origin is available.[5] More recently Wiener[9] has reported unusual frequencies of congenital anomalies of this sort (and including cleft palate, congenital amputations, etc.) among offspring of women whose serum was antagonistic to the fetal blood. He suggests a general serological explanation for malformations, perhaps clogging of fetal vessels. Possibly these loop-tailed mice and their monstrous offspring may serve to test Wiener's hypothesis.

Summary

An inherited but variable syndrome of looped-tail, "wobbly" head movements, and frequently imperforate vagina is described. Outcrosses demonstrate the basis to be a dominant factor with incomplete penetrance. The homozygous condition combines craniorachischisis (cranial hernia, complete spina bifida), and often umbilical hernia, with death at parturition.

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PROFESSOR VANDEL COMMENTS

(*Continued from page 328*)

darwiniens me parait trop souvent rejoindre (en particulier lorsqu'il s'agit des phénomènes de mimétisme) les excès du créationnisme théologique.

2) Le Prof. Dobzhansky applique à ma conception de la vie et de l'évolution le qualificatif de "vitaliste." A mon sens, ce terme me parait mal choisi, car, ainsi que je l'ai clairement exprimé, dans mon livre (p. 84), je rejette, de façon catégorique, l'idée d'un "principe vital" animant les phénomènes biologiques. Je me rallie bien plutôt, sans les accepter d'ailleurs dans leur totalité, aux idées développées par les philosophes de l'"émergence" (Lloyd Morgan; Samuel Alexander); c'est à dire qu'à chaque niveau d'organisation de la matière, des propriétés nouvelles apparaissent, qui font défaut aux degrés inférieurs. On ne saurait donc retrouver les mêmes lois, ni étudier avec les mêmes méthodes, les organisations atomique, moléculaire, vitale, psychique, encore qu'elles soient reliées les unes aux autres par des transitions souvent insensibles.

3) Si je voulais qualifier par un terme propre ma conception de l'évolution, je l' appellerais "embryologique" ou "phychologique," ces deux termes étant d'ailleurs pour moi trés voisins, l'organisation psychique étant le prolongement direct de l'organisation germinale. C'est la raison pour laquelle l'ontogenèse et le psychisme ont fait l'objet des deux principaux chapitres de mon livre (Ch. VII et IX). De même que le germe possède en lui-même tout ce qui est nécessaire au développement de l'adulte, c'est dans l'organisme lui-même qu'il convient de rechercher la source de l'évolution, et *non* dans quelque facteur *extérieur* à lui: milieu (lamarckisme) ou sèlection naturelle (darwinisme).

4) Ma théorie de l'évolution est anthropocentrique, en ce sens qu'il me parait impossible d'obtenir une vue *correcte* de l'évolution, à moins d'y intégrer l'Homme, et non seulement l'Homme envisagé sous ses aspects anatomique et physiologique, mais, l'Homme *total*, c'est à dire l'Homme intellectuel, affectif, moral et religieux. C'est la raison pour laquelle le problème de l'évolution présente, en plus de ses aspects biologiques, des prolongements métaphysiques. Le problème de l'évolution n'est donc pas un problème strictement scientifique, pas plus qu'aucun de ceux dans lesquels l'homme est impliqué.

5) Contrairement à la plupart des théologiens et des philosophes, je considère que l'homme n'est pas un aboutissement, mais un terme de passage. Si l'évolution animale parait aujourd'hui terminée, elle doit par contre se continuer au delà de l'Homme, et se poursuivre par des ètats qui—autant que nous puissions en juger d'après l'évolution passée—s'orienteront vers des formes de plus en plus dégagées de la matière, et par suite de plus en plus libres et spiritualisées.

A. Vandel.

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2) Received nothing (invoiced, ~~xxxxxxxx~~) You checked this address and found it to be correct. Please advise dispatch date of the first 1949 issue to this client. Were any copies returned undelivered by the Post Office? Thank you for a prompt reply.

Export Department